An Extract of Mr. Iſaac Newton's *Letter, written to the Publiſher from* Cambridge April 3. 1673. *concerning the Number of* Colors, *and the Neceſſity of mixing them all for the production of* White; *as alſo touching the Cauſe why a Picture caſt by Glaſſes into a darkned room appears ſo diſtinct notwithſtanding its Irregular refraction: (Which Letter, being an Immediat anſwer to that from* Paris, *printed* N°.96.p.6086. *of theſe* Tracts, *ſhould alſo, if it had not been miſ-laid, have immediately followed the ſame.)*

IT ſeems to me, that *N.* takes an improper way of examining the nature of *Colors*, whilſt he proceeds upon compounding thoſe that are already compounded; as he doth in the former part of his Letter. Perhaps he would ſooner ſatisfie himſelf by reſolving Light into Colors, as far as may be done by Art, and then by examining the properties of thoſe colors apart, and afterwards by trying the effects of re-conjoining two or more or all of thoſe; and laſtly, by ſeparating them again to examine, what changes that re-conjunction had wrought in them. This, I confeſs, will prove a tedious and difficult task to do it as it ought to be done; but I could not be ſatisfied, till I had gone through it. However, I only propound it, and leave every man to his own method.

As to the Contents of his Letter, I conceive, my former Anſwer to the *Quære* about the *Number* of *Colors* is ſufficient, which was to this effect; That all Colors cannot practically be derived out of the *Yellow* and *Blew*, and conſequently that thoſe *Hypotheſes* are groundleſs which imply they may. If you ask, What colors cannot be derived out of *yellow* and *blew*? I anſwer, none of all thoſe which I defin'd to be Original; and if he can ſhew by experiment, how they may, I will acknowledge my ſelf in an error. Nor is it eaſier to frame an *Hypotheſis* by aſſuming only two Original colors rather than an indefinit variety; unleſs it be eaſier to ſuppoſe, that there are but two figures, ſizes and degrees of velocity or force of the Æthereal corpuſcles or pulſes, rather than indefinit variety; which certainly would be a harſh ſuppoſition. No man wonders at the indefinit variety of Waves of the Sea, or of ſands on the ſhore; but,

The Royal Society is collaborating with JSTOR to digitize, preserve, and extend access to
Philosophical Transactions (1665-1678).
www.jstor.org

but, were they all but two ſizes, it would be a very puzling *phænomenon*. And I ſhould think it as unaccountable, if the ſeveral parts or corpuſcles, of which a ſhining body conſiſts, which muſt be ſuppos'd of various figures, ſizes and motions, ſhould impreſs but two ſorts of motion on the adjacent Æthereal *medium*, or any other way beget but two ſorts of Rays. But to examine, how Colors may be explain'd *hypothetically*, is beſides my purpoſe. I never intended to ſhew, wherein conſiſts the Nature and Difference of colors, but only to ſhew, that *de facto* they are Original and Immutable qualities of the Rays which exhibit them; and to leave it to others to explicate by Mechanical *Hypotheſes* the Nature and Difference of thoſe qualities: which I take to be no difficult matter. But I would not be underſtood, as if their Difference conſiſted in the Different Refrangibility of thoſe rays; for, that different Refrangibility conduces to their production no otherwiſe, than by ſeparating the Rays whoſe qualities they are. Whence it is, that the ſame Rays exhibit the ſame Colors when ſeparated by any other means; as by their different *Reflexibility*, a quality not yet diſcourſed of.

In the next particular, where *N.* would ſhew, that it is not neceſſary to mix all Colors for the production of *White*; the mixture of *Yellow*, *Green* and *Blew*, without *Red* and *Violet*, which he propounds for that end, will not produce *White*, but *Green*; and the brighteſt part of the Yellow will afford no other colour but Yellow, if the Experiment be made in a room well darkn'd, as it ought; becauſe the Colour'd light is much weaken'd by the Reflexion, and ſo apt to be diluted by the mixing of any other ſcattering light. But yet there is an Experiment or two mention'd in my Letter in the *Tranſactions Numb.* 88, by which I have produced *White* out of two colors alone, and that variouſly; as out of *Orange* and a *full Blew*, and out of *Red* and *pale Blew*, and out of *Yellow* and *Violet*, as alſo out of other pairs of Intermediat colors. The moſt convenient Experiment for performing this, was that of caſting the colors of one Priſme upon thoſe of another, after a due manner. But what *N.* can deduce from hence, I ſee not. For the two colors were compounded of all others, and ſo the reſulting White, (to ſpeak properly,) was compounded of them all, and only de-compounded of thoſe two. For *inſtance*, the *Orange* was compounded of Red, Orange, Yellow and ſome Green; and the *Blew*, of Violet, full Blew, light Blew, and ſome Green, with all their Intermediat degrees; and conſequently the Orange and Blew together made an Aggregate of all colors to conſtitute the White. Thus, if one mix red, orange and yellow Powders to make an Orange; and green, blew and violet colors to make a Blew; and laſtly, the two mixtures, to make a Grey; that Grey, though de-compounded of no more than two Mixtures, is yet compounded of all the ſix Powders, as truly as if the powders had been all mixt at once.

This is ſo plain, that I conceive there can be no further ſcruple; eſpecially to them who know how to examine, whether a colour be ſimple or compounded, and of what colors it is compounded; which having explained in another place, I need not now repeat. If therefore *N.* would conclude any thing, he muſt ſhew, how White may be produced out of two *Un-compounded* colors; which when he hath done, I will further tell him, why he can conclude nothing from that. But I believe, there cannot be found an Experiment of that kind; becauſe, as I remember, I once tryed, by gradual ſucceſſion, the mixture of all pairs of Un-compounded colors; and, though ſome of them were paler, and nearer to White, than others, yet none could be truly call'd White. But it being ſome years ſince this tryal was made, I remember not well the circumſtances, and therefore recommend it to others to be tryed again.

In the laſt place, had I thought, the Diſtinctneſs of the Picture, which (for *inſtance*) a Twelf foot Object-glaſs caſts into a darken'd room, to be ſo contrary to me as *N.* is pleaſed to affirm, I ſhould have waved my Theory in that point before I propounded it. For, that I had thought on that difficulty, you may eaſily gueſs by an expreſſion, ſomewhere in my firſt Letter *, to this purpoſe; That I wonder'd, how Teleſcopes could be brought to ſo great perfection by Refractions which were ſo Irregular. But, to take away the difficulty, I muſt acquaint you, *firſt*, That, though I put the greateſt Lateral error of the rays from one another to be about $\frac{1}{50}$ of the Glaſſes diameter; yet their greater error from the Points on which they ought to fall, will be but

---

* *See* Numb. 80. p.3079.

but $\frac{1}{100}$ of that diameter: And *then*, that the rays, whoſe error is ſo great, are but very few in compariſon to thoſe, which are refracted more Juſtly; for, the rays which fall upon the middle-parts of the Glaſs, are refracted with ſufficient exactneſs, as alſo are thoſe that fall near the *perimeter* and have a *mean* degree of Refrangibility; So that there remain only the rays, which fall near the perimeter and are *moſt* or *leaſt* refrangible to cauſe any ſenſible confuſion in the Picture. And theſe are yet ſo much further weaken'd by the greater ſpace, through which they are ſcatter'd, that the Light which falls on the due point, is infinitely more denſe than that which falls on any other point round about it. Which though it may ſeem a *Paradox*, yet is certainly demonſtrable. Yea, although the Light, which paſſes through the middle parts of the Glaſs, were wholly intercepted, yet would the remaining light convene infinitely more denſe at the due points, than at other places. And by this exceſs of Denſity, the Light, which falls *in* or inviſibly *near* the juſt point, may, I conceive, ſtrike the *ſenſorium* ſo vigorouſly, that the impreſs of the weak light, which errs round about it, ſhall, in compariſon, not be ſtrong enough to be animadverted, or to cauſe any more ſenſible confuſion in the Picture than is found by Experience.

This, I conceive, is enough to ſhew, Why the Picture appears ſo diſtinct, notwithſtanding the Irregular refraction. But, if this ſatisfie not, *N.* may try, if he pleaſe, how diſtinct the Picture will appear, when all the *Lens* is cover'd excepting a little hole next its edge on one ſide only: And if in this caſe he pleaſe to meaſure the breadth of the colors thus made at the edge of the Suns picture, he will perhaps find it to approach nearer to my proportion than he expects.

An Anſwer (to the former Letter, written to the Publiſher June 10.1673. *by the ſame Pariſian Philoſopher that was lately ſaid to have written the Letter already extant in* N°. 96.p.6086.

—— Touching the Solutions, given by M *Newton* to the ſcruples by me propos'd about his Theory of Colors, there were matter to anſwer them, and to form new difficulties; but ſeeing that he maintains his opinion with ſo much concern, I liſt not to diſpute. But what means it, I pray, that he ſaith; *Though I ſhould ſhew him, that the White could be produced of only two Un-compounded colors, yet I could conclude nothing from that.* And yet he hath affirm'd in *p.* 3083. of the *Tranſactions*, that to compoſe the White, all primitive colors are neceſſary.

As to the manner, whereby he reconciles the effect of Convex glaſſes for ſo well aſſembling the rays, with what he eſtabliſhes concerning the different refrangibility, I am ſatisfied with it; but then he is alſo to acknowledge, that this aberration of the rays is not ſo diſadvantagious to Optic glaſſes as he ſeems to have been willing to make us believe, when he propoſed *Concave ſpeculums* as the only hopes of perfecting Teleſcopes. His invention certainly was very good; but, as far as I could perceive by experience, the defect of the Matter renders it as impoſſible to execute, as the difficulty of the Form obſtructs the uſe of the *Hyperbole* of M. *Des-Cartes*: So that, in my opinion, we muſt ſtick to our Spheric Glaſſes, whom we are already ſo much obliged to, and that are yet capable of greater perfection, as well by increaſing the length of Teleſcopes, as by correcting the nature of Glaſs it ſelf. *So far He.*

To this Letter is to be referr'd that, which is already extant in N.96.p.6087. *as being an Anſwer thereto.*